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## **Background knowledge and the structuralist approach. Reply to Jaap Kamps**

Kuipers, Theo A.F.

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**Theo A. F. Kuipers**

**BACKGROUND KNOWLEDGE AND THE STRUCTURALIST  
APPROACH**

**REPLY TO JAAP KAMPS**

In ICR and SiS I have emphasized the nature of the long-term dynamics of science. For example, in SiS (p. 38) I wrote:

However, the [two-level] picture [of theoretical and observational terms] hides the long-term dynamics. When a proper theory is accepted as (approximately) true, it usually enables the establishment of criteria for the determination of its theoretical terms. In this way it becomes an observation theory, and the corresponding theoretical level transforms into a higher observational level, enabling new observations and hence the establishment of new observational laws, requiring new, ‘deeper’ theories to explain them.

In fact, this long-term dynamics leads to the growth (and occasional repair) of the “unproblematic” background knowledge. As far as scientists are aware of a specific increase in this respect, it concerns *explicit* background knowledge, which can be taken into account when questions of implication and hence falsification or confirmation are concerned. However, as Jaap Kamps argues first in general and then by way of a very nice “Tarski world” example, implicit background knowledge is something we have to excavate and computational means can be very helpful for that purpose.

I find his exposition very elegant and convincing, so I will only deal with some points raised in the last section where he presents his conclusions and discusses them. The first point deals with the question whether making true background knowledge explicit is a form of truth approximation. The second point concerns the advantages and disadvantages of the statement and the structuralist approach.

**Truth Approximation by Adding True Background Knowledge**

In the second paragraph of Section 5 and Note 9 Kamps discusses the effect of adding true background knowledge for a model as well as a statement or, more specifically, a consequence approach to truth approximation. From additional

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correspondence it became clear that for the first he has primarily the basic definition of ICR in mind and for the second Popper's original definition. It turns out to be interesting to elaborate Kamps' main points about them in some detail.

According to the (basic) model definition, ' $\psi$  is at least as close to the truth as  $\phi$ ' iff:

all correct models of  $\phi$  are (correct) models of  $\psi$

all incorrect models of  $\psi$  are (incorrect) models of  $\phi$

where a model is correct iff it is a model of the truth, that is, the strongest true theory about the intended domain within a given vocabulary.

Popper's consequence definition has a similar form, in brief:

all true consequences of  $\phi$  are (true) consequences of  $\psi$

all untrue consequences of  $\psi$  are (untrue) consequences of  $\phi$

It is not difficult to prove that, whereas the first consequence clause is equivalent to the second model clause, the second consequence clause is essentially stronger than the first model clause. For further details, among other things, about underlying intuitions, see ICR Section 8.1.

Now let us see what adding background knowledge amounts to, according to the two definitions. Adding  $\beta$  to a theory  $\phi$ , results of course in a stronger theory,  $\psi = \phi \& \beta$ . Since all consequences of  $\phi$  are consequences of  $\phi \& \beta$ , or equivalently, all models of  $\phi \& \beta$  are models of  $\phi$ , we get, in line with the abovementioned equivalence, that the second model clause as well as the first consequence clause are automatically satisfied.

If  $\beta$  is true,  $\phi \& \beta$  will drop only incorrect models of  $\phi$ , hence the first model clause is satisfied, hence  $\phi \& \beta$  is at least as close to the truth as  $\phi$ , and, we may add, as a rule, closer to the truth, according to a plausible extra condition. On the other hand, as Kamps rightly hints upon in Note 9, if  $\beta$  is true,  $\phi \& \beta$  may not only have extra true consequences, but also extra untrue ones. Hence, the second consequence clause is not guaranteed. Therefore,  $\phi \& \beta$  need not be as close to the truth as  $\phi$ , let alone closer to the truth. Of course, if  $\phi$  is also true,  $\phi \& \beta$  has only true extra consequences compared to  $\phi$  (and  $\beta$ ). In this case Popper's definition even guarantees truth approximation, and the model definition does too.

Kamps presents the diverging conclusions, evidently assuming as a condition of adequacy for a definition of 'closer to the truth' that adding true background knowledge should always leave us as close to the truth and, as a rule, bring us closer to it. In other words, excavating background knowledge should, if true, be functional for truth approximation.

More generally, I would like to submit as a general condition of adequacy for a ‘content definition’ (to use the apt expression of Zwart (1988/2001) for the type of definitions we are discussing now) of ‘closer to the truth’ that adding some true statement (or its model equivalent; dropping incorrect models) should be functional for truth approximation in the indicated sense. From the above it follows that the model definition satisfies this general condition, whereas Popper’s definition fails to do so.

I am particularly eager to point to this condition for the following reason. The famous impossibility theorem against Popper’s definition, independently proved by Miller and Tichý, typically assumes that “the truth” is complete, due to having one intended model. In that case, a false theory cannot be closer to the truth than another theory according to Popper’s definition (see ICR Section 8.1 for a detailed reconstruction). In view of my general belief that the truths that one looks for in theoretically oriented empirical sciences are incomplete, one might say that the impossibility theorem should not be that impressive, for it applies only in an extreme, atypical case. However, the general condition of adequacy proposed above for content definitions, in the line of Kamps’ discussion of background knowledge, provides an argument against Popper’s definition and in favor of the model definition that also applies to paradigmatic cases of theory improvement: adding true statements about the domain of interest should never be counterproductive for truth approximation but, as a rule, productive.

The important question remains whether the refined definition of truth approximation presented in ICR (Ch. 10, p. 250), being a likeness definition in the sense of Zwart, satisfies the general condition of adequacy. Since the refined second model clause is a weakening of the corresponding basic one, it is again automatically satisfied for any added statement. However, the refined first model clause is a strengthening of the corresponding basic one. For this reason, the refined one need not always be satisfied when a true statement is added. More specifically, adding a true statement  $\beta$  to  $\phi$  (leading to  $\phi \& \beta$ ) does not exclude the possibility that all incorrect models of  $\phi$  get lost that could be the “intermediate” model, required by the refined first clause, between a given incorrect model of  $\phi$  and a given intended model, both being no model of  $\beta$ . It is a question for further research whether this should be seen as a really problematic aspect of the refined definition, as Kamps probably thinks, or whether the discussed condition of adequacy is too ambitious or even undesirable for likeness theories in general.

### The Logical Versus the Structuralist Approach

Regarding the advantages of the structuralist approach, I have certainly overstated my claim in one respect, viz. “that the structuralist analysis of theories can be used almost directly for the computational representation of theories” (SiS, p. 302). It is rightly criticized by Kamps in Note 13, though perhaps for the wrong reason. It is not so much that you need a “pencil, paper, and a philosophy professor,” but that for computational purposes you need some kind of syntactically tractable transcription of all the relevant set-theoretical aspects. My suggestion that this is already (almost) always possible is far from the truth. However, in many cases such a transcription is indeed possible, as in the case of many sociological theories, but also in Kamps’ nice example of Tarski’s world. See also Balzer and Moulines (2000, p. 9), for general optimism with respect to implementing set-theoretic representations in AI.

Apart from the computational claim then, I would insist on the general claim that for many purposes the structuralist representation is less complicated than a logical one. To begin with, above we have seen that it is possible to give a logical definition of ‘closer to the truth’ – in fact three equivalent versions are possible: the given version purely in terms of models, a complicated version in terms of consequences, and a dual version, combining the first clauses of the two definitions discussed above. However, the core of all three versions can easily be reproduced completely in set-theoretic terms (ICR, pp. 184-6), viz. by replacing models by structures of a certain type and conceiving theories as sets of structures and, when desired, their (set of) consequences as (the set of) supersets of these sets. As a matter of fact, I invented that definition by starting, in 1982, to think in the latter way (ICR, pp. 150-3), more specifically, the one purely in terms of theories as sets of structures (corresponding to the purely model formulation), and hence first avoiding all complications in surveying and comparing the sets of true and false consequences of theories.

Another nice example of structuralist representation is suggested by Kamps’ own illustration. If Tarski’s world were not designed for didactic logical purposes, but to illustrate the nature of empirical theories, assuming that some serious empirical law would be involved, see below, the set-theoretic representation would be superior, not in principle, but in (non-computational) practice, for a couple of reasons. As a matter of fact, it is an elementary exercise to give the set-theoretic representation (see SiS, Ch. 12) of what then would plausibly be called “Tarski Worlds,” for example Kamps’ counterexamples I to IV. A Tarski World is a set-theoretic structure of a certain type, e.g. with one base set (domain) and a number of unary, binary and

ternary relations, satisfying a number of analytic or semantic axioms and a number of synthetic or substantial axioms. For example, let  $D$  indicate a domain of objects and  $C$  the subset of cubes,  $T$  the subset of tetrahedrons,  $S$  ( $L$ ) the subset of small (large) objects. Analytic (background) axiom B3 amounts to  $S \cap L = \emptyset$  and synthetic axiom A1 amounts to  $D - C \cup (T \cap S) = \emptyset$ . Of course, these clauses can be transcribed in first-order claims, as Kamps has done, but for many purposes the former are just simpler than the latter. Unfortunately, A1 is not at all like an empirical law, but within the present boundaries one might think of a condition to the effect that a cube cannot be positioned on top of a tetrahedron. Although this is conceptually possible, we may assume that it would fall to the ground. It would be instructive to transform the example into a more serious example of similar structures of a physical theory. To be sure, for Kamps' computational purposes, some syntactic redescription is required. For that purpose one should first look for a first-order redescription, for if that is possible, as in Kamps' case, programs like OTTER and MACE can be used.

Let me close by referring to the contributions of Zwart, Van Benthem, Burger and Heidema, and my replies in the companion volume. Among other things, the comparison of the logical versus the structuralist approach is discussed as well as the desirability of an "alternative model theory" that is more suitable for the structuralist approach.

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